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Interactive comment on “A global database of soil respiration data” by B. Bond-Lamberty and A. Thomson

Anonymous Referee #4

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The database presented in this manuscript represents, undoubtedly, a very need effort. I have, however, some criticisms of the lack of information about organization of the data-base and how the author's are planning to deal with some, to me, important issues not well addressed in the present manuscript.

The discussion about quality control is a bit poor. How the author's are going to deal with quality control? At which scales? What are those histograms for the primary variables? There are some methodological errors that author's should at least mention because one of the many objectives of this effort will be to scale up local fluxes to understand global trends and calculate global totals. Hence, methodological errors such as those associated to the technique used (solid-state, closed dynamic, closed static) or the instrument used (Li-COR, pp-Systems, etc....see Pumpanen et al for

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same hints) would be potentially important errors worth to discuss.

The issue of spatial variability is a tricky one as the author's mention. More than at the macro-scale, I am especially concern about the bias associated to the large spatial variability at the plot-scale (meso and micro-scale). For instance, one of the questions that came to my mind while reading the manuscript was if the data would be classified according to the reliability at spatial scale. I think that this is an important issue the author's should at least discuss this potential bias.

How the author's are going to deal with ancillary data collection?. I think the author's should briefly discuss about a ranking of important ancillary data, e.g. temperature, precipitation and productivity will be probably most important, but also soil litter layer depth, organic carbon content in mineral soil, NPP; also including some potentially important data, such as site diversity.

There is no discussion about how the database is going to deal with different management regimes, e.g. . Since most of the land is managed, I think that it is time to start including management effect on fluxes

Some other things I missed were:

Why isn't it plotted Rs versus temperature, moisture and/or LAI? It's in the discussion but

I also miss some methodology on the calculations shown in the manuscript; e.g. How were the average fluxes calculated? Using weighted means? Just raw means?

The paragraph 2.1 is a bit confusing. Not clear whether the database presented in the manuscript included only data from 2008. The author's, however, mention the data collected in other papers, such as Hibbard et al. 2005. I am not sure if some Rs from those articles is included. I am sure that not all the sites from this studies, actually data that fulfill the inclusion criteria explained by the authors. Could you explain better this?

BGD

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